NELSON GOODMAN

REPLIES

10. REPLY TO ESSLER

I have no quarrel with Wilhelm K. Essler's paper, and little to add. He quite correctly stresses the fact that learning from experience involves not only examining data but deciding what predicates are to be used in formulating both data and hypotheses. Simplicity is one prominent factor in this choice and, far from amounting merely to a consideration of elegance, is an ingredient in inductive validity.[1] To a considerable degree, furthermore, simplicity depends upon habit, upon entrenchment; and predicates that are of equal simplicity by any formal criterion may differ drastically in the simplicity that guides selection for inductive use.

To say this is, indeed, to take issue with traditional formulations of empiricism, but it by no means favors the usual current irrational versions of rationalism. Resting the matter on habit, on precedent, on what may be historical accident, is a far cry from resting it on ideas universally stamped on the mind at or before birth. One might try to show that the selected predicates have a characteristic that relates to some discoverable physiological or neurological characteristic. The trouble is that description of the characteristics of the predicates and of the organism will depend upon a similar choice among the terms of description. Still, to define simplicity in terms of entrenchment, of an accident repeated over and over, neither precludes nor requires a further explanation of the initial accident from some empiricist or rationalist or other orientation.

NOTE

[1] In addition to the references Essler gives, see *Problems and Projects*, pp. 334–370.

Erkenntnis **12** (1978) 281–291. *All Rights Reserved*

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11. REPLY TO KUTSCHERA

With Franz von Kutschera's paper, in contrast, I have several quarrels and, if little to add, something to subtract. The quarrels concern various objections, most of them already made by others and already answered in some of my papers.[1] Let me answer again briefly before considering the main argument of his paper.

1. He charges that use of the notion of conflict in my treatment of projectibility is 'flatly circular'. But what I have set forth is an answer to the question which of two conflicting hypotheses, under specified circumstances, is projectible. We ask whether H_1 or H_2 is projectible only if we assume or suspect conflict between them, but the rules for making the decision in no way depend on that assumption being correct. If that assumption is wrong, which hypothesis we choose or whether we choose doesn't matter. In sum: if H_1 and H_2 conflict, then (e.g.) H_1 but not H_2 may be projectible; if they don't, then both are projectible or neither is, and choice between them is indifferent. Of course in developing and illustrating the rules of choice we focus on cases where conflict is assumed. There is no circle here, flat or otherwise. Rather, Kutschera's reasoning is pretzel-shaped.

2. Kutschera writes that another circle is involved in comparing the entrenchment of predicates, since we must consider other predicates with the same extension, and whether two predicates are coextensive is a matter for inductive inference.[2] Now indeed we may be wrong in judging coextensiveness; and if so our judgment about the projectibility of a hypothesis may be wrong. But the rules for projectibility are not intended to guarantee that decisions we intend to be in accord with them will be correct; our decisions may fail to be in accord with the rules. *If*, all else aside, H_1 is much better entrenched than H_2, and the two conflict, then H_1 is projectible and H_2 is not. This is unaffected by mistakes we may make in judging either comparative entrenchment or conflict. Kutschera seems to suppose that defining inductive validity or justifying induction requires providing a rule that guarantees its own correct application.

3. Kutschera repeats the old Kahane complaint that my treatment of projectibility in terms of entrenchment would prevent the introduction of new predicates into science; that "radioactive", for example, would have been barred as initially no better entrenched than the obviously

inadmissible grue-like “radiotractive”. This ignores the entrenchment that the former inherits from parent predicates, and the weight that hypotheses on the predicate acquire from overhypotheses. By such means, indeed, “radioactive” gains the advantage from the very association that it claims with terms like “green” and “blue” through its nonpositionality with respect to them and in terms of our antecedently well-entrenched vocabulary. Absolute nonpositionality is an empty notion; but relative to predicates already classed as projectible, nonpositionality becomes a legitimate avenue for the inheritance of entrenchment.

I pass over the remaining equally familiar and answerable objections to consider Kutschera’s main argument. So far as I can see, it begins with a rejection of my conception of the genuine problem of justifying induction and ends with complete acceptance of it.

He announces near the start that “we are not interested in the problem as to whether . . . inductive arguments are in conformity with how people usually argue. Our concern is rather . . . whether they prove what they purport to prove. If they do we need not bother about customary usage, and if they don’t, such arguments would be ineffective even if they would conform to ordinary practice.” And later “we shall not adopt Goodman’s descriptive use of the terms ‘justification’ and ‘valid’ but rather employ them in their normal logical sense”; and still later, he emphasizes that the real problem is not “how to describe actual inductive practice” but rather “whether and in what sense inductions are *sound*”.

The answer he offers is in terms of subjective probability, which he describes as “the degree of certainty with which the person X expects A to happen”. Further the “axioms are analytical postulates for a rational concept of subjective probability”. But lest this suggest that rational belief is just normal expectation, he insists “the concept of subjective probability is not purely descriptive, as we have seen [!], but a concept of rational belief”. Yet later he writes, “Hume has shown once and for all that there is no rational basis for prophecy.”

The reader confused by all this may take what comfort he can from the dictum that soon follows: “to follow probabilities where certainties are not available is surely an indispensable postulate of rationality. And that we *expect* to be successful if we follow our expectations is as evident as any tautology.”

In my view,[3] inductive validity is constituted by a codification of normal behavior; justification of a particular inference amounts to showing its conformity to the rules in that codification; and justification of the rules amounts to showing their agreement with normal expectations. Rules and judgments may be brought into accord by adjustment of either. I have been unable to discern how Kutschera's view differs appreciably from that.

NOTES

[1] See for example *Problems and Projects*, pp. 357–370 and 398–412; and later papers in *The Journal of Philosophy*.

[2] See one of my summaries of the fallacy in this objection in *Problems and Projects*, p. 360.

[3] Cf. *Fact, Fiction, and Forecast*, pp. 62–66.

12. REPLY TO HELLMAN

Geoffrey Hellman discusses a matter that has gained increasing attention lately: the criterion of constructional definition formulated in Chapter I of *The Structure of Appearance.* The strong support it provides for a radical relativism has converted some former absolutists and has sent others back to rebuild their bastions.

I distinguish the general apparatus (loosely called the "logic") of a system from its special 'extralogical' terms. The general apparatus consists of basic logic together with the calculus of classes based upon membership and the calculus of individuals based upon overlapping.[1] The distinction between the general apparatus and the rest of the system is defined only by enumeration of the primitives of the general apparatus; but the principles guiding the enumeration are two. First, the general apparatus is common to all the systems under consideration; that is, each contains the basic logic and either or both of the calculi. Second, the general apparatus is schematic; something that under one system may be a part or member of another may under a different system be a whole or class containing that other. Varied embodiments leave the structure of the calculi unaltered.

Now the definitions that concerned me were all 'extralogical': introduced after the general apparatus had been fixed, and making use of at least one of the special primitives of the system. For such constructional definitions I developed a criterion in terms of extensional isomorphism, not at all meant to apply to definitions entirely in terms of the general apparatus. Thus most of Hellman's discussion in the early part of his paper has little to do with my treatment of constructional definition. His examples – the definitions of numbers and of ordered pair – are purely in terms of set theory and raise no questions concerning my criterion in its intended application.

Of course, one may seek a criterion for definitions lying entirely within the general apparatus. When Hellman comes to the definition of ordered pair, he concludes that no isomorphism criterion will work. Earlier, however, he makes a proposal for definitions of numbers that he considers "clearly within the spirit of" my criterion but that actually, I think, already forsakes isomorphism. Let me explain.

My formulation requires of the definitions of a system that there be some consistent way of replacing the ultimate factors subsumed by the extensions of the definienda so as to yield the extensions of the definientia. An ultimate factor is an individual or the null class; and replacement is consistent if the same replacement is made throughout for each such factor, different replacements for different factors, and the null class replaces itself. The set of definiens-extensions will thus reflect all such relationships as identity, nonidentity, inclusion, intersection, ordering, etc. in the set of definiendum-extensions and, since the replacements need not be ultimate factors, may introduce additional relationships.[2]

Hellman wants to relax my requirements just to the extent of allowing any replacement for the null class that does not also replace another ultimate factor. But now suppose our definienda are names of the disjoint classes *A* and *B* and of the null class *C*.[3] Then *C* is included in both *A* and *B* and no replacement for *C* other than *C* itself will preserve these inclusions. Allowing *C* to be replaced by something else thus sacrifices assurance of even the minimal nonsymmetric isomorphism that provides the rationale for my criterion of definition.

In his third section, Hellman rather surprisingly argues that to save materialism, the criterion of definition must be strengthened rather than weakened. How can it be that a tighter, more restrictive criterion will accommodate a theory that a looser, less restrictive one will not? The basic answer, I think, is that the materialist is at heart an absolutist; he demands not merely that his own program be accepted but that alternative nonmaterialistic programs be banned. Our retreat from intensionalist and extensionalist identity criteria had the effect, indeed the purpose, of admitting for instance definitions of points in terms of lines as well as of lines in terms of points, of admitting both realistic[4] and particularistic systems, physicalistic and phenomenalistic systems, etc. But a materialism that tolerates a mentalistic alternative has lost its heart – or its spleen.

Under the isomorphism criterion, the set of definiens extensions must reflect and may elaborate the structure of the set of definiendum extensions but need not reflect cross-connections between definiendum and definiens extensions. For example, consider two perpendicular pairs of parallel lines, intersecting at four points. In one system, the lines are defined as pairs of collinear points; in another, the points are defined as pairs of intersecting lines. Definitions meeting the isomorphism criterion can be developed in either system for the various relations between the lines and between the points and even between the lines and the points; but the criterion is entirely noncommittal on the question whether a point 'really' is a member of a line or a line a member of a point. This obviously will not satisfy anyone who takes a firm stand on either side of that question. Likewise, neither materialists nor mentalists can abide such neutrality between their programs.

How, then, does Hellman proceed? He requires that while the structure *within* the set of definiendum extensions need only be reflected in the set of definiens extensions, certain relations between definiendum and definiens extensions must be *absolutely* preserved. In terms of our line and point example, this might mean for instance that membership of the points in the lines must be retained by the definitions as membership of the points in the lines. To say that a relation is absolutely preserved is of course to make a commitment – e.g. to say that the points are really members of the lines and not vice versa. Hellman does not directly require absolute preservation of the composition of the mental out of the material, but rather requires absolute preservation of some relations such as precedence between a physical and a mental event. At first sight, this seems to run counter to his purpose since absolute preservation of precedence between mental and material events would seem to preclude anything like definition of either in terms of the other; but I gather that Hellman hopes that from the relations he requires to be absolutely preserved he can somehow render the mental as superfluous for materialism as lines are for the biased advocate of the point-based system of our example.

Hellman and I are in full agreement, however, that the supposed paradox or threat of 'mathematicism' or neo-Pythagoreanism is utterly empty. The standard for judging a system – that is, what structure must be preserved, absolutely or relatively – is decided outside the system. If we decide that a system must recognize that neither do points consist of ines nor lines of points, then both of our sample systems are ruled out. and if we decide that numbers are not identical with physical objects and that a system must preserve this non-identity, then mathematicism is ruled out. A system cannot overrule but is governed by such a decision.

The rest of Hellman's paper is concerned with seeking a possible or feasible (i.e. not obviously untenable) thesis of materialism. Although he explored adjustment of the criterion of definition to accommodate materialism, he says that materialism need not be constructional – that the slogan "no difference without a physical difference" may be taken to mean not that all differences are definable from but only that all are 'determined by' physical differences. That raises the question what constitutes 'determination'; and neither Hellman nor anyone else so far as I know gives more than vague hints at an answer. One suspects furthermore that an interpretation general enough to license a materialistic system may also license an alternative mentalistic system. All the same, an unrepentant relativist must be impressed by how conviction here hunts for a doctrine, how the word "materialism" inspires loyalty to a thesis still wanting not only substantiation but even clear formulation.

NOTES

[1] Obviously, other terms of these calculi may instead be taken as primitive; and the 'enumeration of primitives' mentioned in the following sentence must allow for such alternatives.

[2] e.g. $\{a, b\}$ and $\{b, c\}$ might replace f and g, introducing a new intersection.

[3] Since the null class is a logical constant, this example may be oversimple, although a predicate chosen as an extralogical primitive might in fact have null extension. However, the difficulty illustrated here will also turn up in more complex cases where the null class is a member of a non-null extralogical class.

[4] "Realistic" and the other terms for types of system in this sentence are used in the way defined in *SA*, IV 4, 5.

13. REPLY TO BREITKOPF

In *The Structure of Appearance*, several different calculi are sketched, with primitives specified and definitions stated, but usually with little attempt to prove the sample propositions listed or even to sort them into postulates and theorems. Alfred Breitkopf has performed a useful service in systematizing four of these calculi. Whether or not his formulations are the most economical or are optimal in other respects, they provide a basis to work from.

Breitkopf's systematization rests on four primitive predicates and twenty postulates. Some savings over this are surely possible. For example, his postulates 2.4 and 4.1 follow immediately from substitutivity of identity. Again, postulate 3.6 is rather easily proved from the other postulates; and this suggests that some of the other postulates might be turned into theorems.

As for the primitive predicates, Breitkopf ignores the derivation in *The Structure of Appearance* (pp. 282–285) of three of these to one. Togetherness ("W"), size equality ("Z" in my notation, "$\equiv$" in his),[1] and matching ("M") can all be reduced to a single, two-place, symmetric, reflexive relation of alliance ("A"). By slightly complicating the definitions of the three erstwhile primitives, many of the other postulates become provable.

Of course, as Quine and I have shown,[2] *all* such postulates can always be definitionally eliminated. The result often seems highly artificial; but since no one has succeeded in distinguishing such artificial from genuine reduction of postulates, the whole notion of postulational economy is called in question. This does not keep us from seeking what seems by intuition maximum normal economy.

Breitkopf's development of the calculus of individuals (overlapping) is comprehensive and based on a well-chosen and spare set of postulates. Here and throughout he presents the formal system without comment, but we may well remind ourselves that this calculus, belonging to the 'general apparatus' of systems (see *The Structure of Appearance*, pp. 33–34), contains no postulate that guarantees either that there are only finitely or that there are infinitely many individuals. A postulate guaranteeing infinity – $(x)(\exists y)(y \ll x)$ – is at hand; but what seems like an equally available postulate of infinity – $(x)(\exists y)(x \ll y)$ – is inconsistent with the rest of the calculus. A postulate guaranteeing that the number of individuals is finite cannot even be formulated within the calculus,[3] but may of course be imposed in terms of other primitives.

While many steps in Breitkopf's formalizations were quite easy to make, some proofs have obviously taken great trouble. See, for example, his long proofs of his 2.58 (my 7.611), which I had carelessly called easily provable (*The Structure of Appearance*, p. 241), and of his 3.26 and 3.27, which conjoined are equivalent to my unproved 8.27 (*The Structure of Appearance*, p. 255).

Breitkopf's size calculus, indeed, is developed from the mere fragment offered in my book. Again, it is presented without comment. He does not point out the salient fact that this calculus does require finitude. Since an individual is explained as of the same size as another if and only if the two have the same number of atomic parts, any two individuals consisting of denumerably many atoms would be of the same size. Yet contrary to 3.9 (*The Structure of Appearance* 8.23), one might be a proper part of the other. Thus adoption of our primitive and postulates here precludes infinity of the universe of discourse, blocking addition of even the hitherto admissible postulate $(x)(\exists y)(y \ll x)$.

NOTES

[1] Unfortunately, his logical and extralogical symbols sometimes differ from mine in ways that are likely to confuse; for example, he uses "K" in place of my "¢", and "S" in place of my "K".

[2] "Elimination of Extralogical Postulates", *Journal of Symbolic Logic* **5** (1940), 104–109. Reprinted in *Problems and Projects*, pp. 325–333.

[3] This result, and that of the preceding sentence are due to David Lewis and Wilfrid Hodges, "Finitude and Infinitude in the Atomic Calculus of Individuals", *Nous* **2** (1968), 405–410. Subsequently, Geoffrey Hellman, in "Finitude, Infinitude, and Isomorphism of Interpretations in some Nominalistic Calculi", *Nous* **3** (1969), 413–425, showed how the calculus might be so restricted, if desired, as to admit postulating upward infinity without contradiction.

14. REPLY TO FOX

The development of order on the basis of matching or indifference in Chapters IX and X of *The Structure of Appearance* has attracted the attention of mathematicians, psychologists, and social scientists. The attraction lies in the promise of obtaining significant orderings and measurements from a basis that is logically and psychologically simple and scientifically neutral. The two-place, symmetric, reflexive predicate "matches" has a lower complexity-value, on a well grounded scale,[1] than such more familiar predicates as "brighter than", "is a pair more alike than the pair", etc. And judgments of color matching, for example, unlike comparisons of brightness, do not presuppose any particular analysis of colors into components. Just because matching provides so meagre and neutral a basis, derivation of a full theory is a difficult and fascinating mathematical task.

The problem, the purpose, and the progress to date has been reviewed informally in my paper "On Order from Indifference" written especially for *Problems and Projects* (pp. 423–436). Nonmathematicians may want some such introduction to Ivan Fox's paper.

Insofar as simple open linear arrays alone are concerned, the theory of order and measure on the basis of matching has been virtually complete for some time; but Fox here works out a much more careful, consecutive, and thorough organization of that theory than is available elsewhere. And this, I understand, is the first part of a comprehensive study that covers nonlinear arrays as well and deals with many complex questions that have so far remained open.

Any critical review of Fox's paper must be left to someone mathematically more competent than I.

NOTE

[1] *The Structure of Appearance*, III, 8 and IX, 2.

Manuscript received 23 April 1977